# THE CONCEPTION AND TESTING OF THE RIVER CONTINUUM CONCEPT

C.E. Cushing

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Battelle-Pacific Northwest Laboratories
Richland, WA 99352

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Many people, over the years, have asked various questions concerning how the RCC got started, how it developed and so forth. So when I asked several folks during the meeting at Calgary last year if they thought that a historical aspect of its conception and testing would be of interest to the NABS membership, I received a considerable amount of encouragement. It certainly isn't in the category of "if you ignore history, you are doomed to repeat it"; I can't believe anybody else would want to try this! But, a little history doesn't hurt anybody.

So what I would like to do is to convey to you something about how the RCC was conceived and how it was initially tested , thus hoping that this may provide some background to help you better understand a topic that, one way or another, and whether you agree with it or not, has probably affected most of you or at least caused you to think about streams from a broader perspective. It will also give me a chance to give credit to several folks who largely went unrecognized.

## I - Conception

Three meetings, attended by various combinations of folks that became the PIs of the RCC, were held between 1969 and 1972 and essentially started the chain of events that lead to our initial proposal. The first was a AAAS Symposium in Boston organized by Ken Cummins at which four of us presented papers. The second was a Rockefeller funded meeting held at the Stroud Water Research Center, and the third was held at Oregon State U. where we heard from some people purported to be the leading thinkers at the time in terms of stream ecosystem theory. It was following this meeting that we decided that we could work together and do a better job explaining how stream ecosystems worked than

what we had been hearing. As Jim Sedell put it after hearing their ideas, "The Emperor has no clothes."

The first formal meeting was held on a cold and foreboding, perhaps prophetic, day at the Gull Lake Laboratory of Michigan State University in December 1973, with representatives from the five sites: Stroud Water Research Center, Michigan State University, Idaho State University, Battelle-Pacific Northwest Laboratory, and Oregon State University. We had heard that NSF was receptive to a group proposal and we spent a couple of days mulling over ideas for comparable studies that we could do at each site, and, frankly, didn't accomplish much.

We decided to meet again at which time we would summarize the state-of-the-art of different aspects of stream ecology and see if that would lead to something that could be turned into a group proposal. We each agreed to prepare a "white paper" - Wayne Minshall and Robin Vannote would address invertebrates, Ken Cummins would review detritus, Jim Sedell would examine geomorphology, and I would summarize primary productivity.

We next met at Stroud in May of 1974, only this time we invited a larger group to have more minds to pick. We presented the white papers and were still looking for some commonality that we could address at each site. It was during this meeting that Robin Vannote presented his observations on physical dissipation of energy along a continuum - ideas he had developed during his time spent walking streams with hydrologists and geomorphologists.

This provided us with an analog to develop to see if biological communites exhibited some kind of similar pattern in terms of community structure, energy resources, and so forth. We developed some initial ideas and outline for a proposal, returned to our home institutions, and devoted considerable time considering what it was we wanted to measure along a stream continuum.

The PI's then met at Gull Lake in July of 1974 and spent a week during which we finalized our conceptual ideas and decided on the communities, processes, and variables that we thought most appropriate. From the five sites represented, we selected four which represented different biomes, thus maximizing the diversity of our measurements. These were PA, MI, ID, and OR; the streams in southeastern Washington were eliminated because the Yakima duplicated Idaho, the Columbia was too big, and the endorheic spring-streams did not represent a continuum. We determined the responsibilities of the PIs and a post-doc coordinator, wrote the proposal, and shipped it off to NSF.

Then came the shock - we got funded and had to ask ourselves, "Now what do we do?"

We called another meeting, which was held in Pocatello in July of 1975 , at which time we got to work honing our study methods and developing the logistical aspects of the studies. We brought on the post-doc coordinator, Bob Petersen, to handle the full-time, day-to-day activities of coordinating the work at the different sites. We also discussed and adopted most of our sampling protocol, sampling sites, sampling times, etc.

However, it was during this time that we discovered two areas of sampling that concerned us, given the variety of sampling sites involving four biomes and several

stream orders. These were: How do we measure stream community metabolism (PR) and how do we sample detritus or POM?

We decided to hold two workshops to develop comparable or suitable methods for sampling PR and POM. The first was a PR Workshop held at Stroud in August 1975, where representatives from each site involved in the community metabolism measurements gathered to evaluate various chambers and methods at the same site in White Clay Creek under the same conditions. We compared the Oregon State chambers, the Michigan State chambers used by Donna King, and the Stroud stream-side chambers. Further, we compared two open-stream methods: the diurnal $O_2$ change method that Jim Brock used in Idaho, and the diurnal pH change method used at Rattlesnake Springs in Washington. From these studies (Bott et al. 1978) we determined that the OSU chambers best fit most, if not all, situations and selected them to use at all sites.

The second workshop to sort out our POM techniques was also held at Stroud in December 1975. Here we compared notes on the Stroud nested nets, the Idaho box sampler, and the Oregon bomb sampler, and eventually concluded that as long as we used comparable mesh sizes, it didn't matter which type of net holder was used. The key was to get the necessary amount of water through the designated mesh sizes to obtain the size categories we wanted.

We also ran two tests at this time to determine where to sample the stream cross-section in order to provide the "one best point" to collect TOM. One test used a carbon slurry, and the second used diatoms swept from the rocks to provide a slug of suspended particles to determine where the point of greatest concentration occurred in the cross-section. Incidentally, these tests almost cost us the grant; it was bitterly cold and Tom Callahan from NSF was visiting the site and somehow got the only pair of boots that leaked, which resulted in some frozen toes and muttering about future funding.

In each test, samples were collected by depth and across the stream, filtered, and the filters examined visually. These were obviously "quick and dirty" tests that were done on the spur of the moment during our deliberations and were referred to as the Great Carbon Filter experiment. Essentially we found that sampling at about one-third the depth in the thalweg was the best point at which to obtain the most representative sample.

These were the major milestones in the conception, preparation, and development leading to the field work, which is described below.

II - Testing

Sampling began during the fall of 1975 on the low-order streams in the continuum in each biome, and lasted until 1977, with the bulk of the data collected during 1976; these data are reported in Minshall et al. (1983)and Bott et al. (1985) and several site-specific publications. People instrumental in performing these field studies included (alphabetically and with apologies to those unintentionally omitted) Bernie Anderson, Roger Baumgartner, Tom Bott, Cindy Dunn, Dave Funk, Al Graham, Floyd Ritter, Bernie Shaw, and Barb Vannote in Pennsylvania; Virginia Hold, Bob and Donna King, Michael Klug, David Mahan, Roger Ovink, Bob Petersen, Lena Petersen, Bob Speaker, George Spengler, and Milt Ward in Michigan; Jim Brock, Dale Bruns, Tom Cuffney, Tom LaPoint, and John Moeller in Idaho; and Norm Anderson, Stan Gregory, Chuck Hawkins, George LienKaemper, Karen Luchessa, Dale McCullough, Bob Naiman, and Frank Triska in Oregon. Al Scott from Battelle helped during the SCUBA sampling of the large river sites.

During this initial period, we found that some aspects of our original concept needed adjustments. One of these was the matter of tributary inputs and resetting by smaller streams entering large rivers. We wanted to examine this aspect in a stream as free from anthropogenic impacts as possible and eventually selected the Middle Fork of the Salmon River. During a 10 day float trip, we sampled above, below, and in three major tributaries, Indian Cr., Loon Cr., and Big Cr., measuring POM, functional group composition, and algal abundance; these results were published in Bruns et al. (1984) and Cushing et al. (1983).

Following the sampling of low-order streams, we next devoted the efforts of the personnel from all sites into measurements of a single high-order stream. After considerable evaluation during which we considered such streams as the White River, the Buffalo River, and the Yellowstone River, we selected the lower reach of the Salmon River. This was a trade-off between the negative aspects of inaccessability to the middle section (River Of No Return) and severe winter conditions, against the positive aspects of having continuity of data from sampling in low-order reaches earlier in the study and the low incidence of human influences. These results are published in (Minshall et al. (1992). Folks from all four sites gathered together when we made these extensive sampling runs where we established our working headquarters for this effort at a dude-ranch and set up a lab to process samples. It was during this period that we developed and used techniques suitable for large, deep rivers, such as SCUBA, dome samplers, and other methods.

## III - Summary and Legacy

Thirty-three papers in the River Continuum Series have been published in journals, and the original paper by Vannote et al. (1980) has been selected as a Citation

Classic - as of May 1994 it had been cited over 500 times in refereed journals, and I'm sure many more in the literature not included in these tabulations.

This body of work has led to new research by part of our original group. We originally predicted that the FPOM/CPOM ratio would increase downstream, but have found it is not a simple case of CPOM from the headwaters being progressively diminshed in size as it moves downstream. We are currently studying the dynamics of FPOM using $^{14}C$-tagged FPOM (Newbold et al., 1992; Cushing et al. 1993). The original work has, obviously, fostered considerable research by others.

The RCC is currently undergoing worldwide testing in a book to be published soon by Elsevier Press where 23 chapters will examine the usefulness and conformity of the RCC and other stream ecosystem concepts in different geographical areas of the world. Here, and elsewhere, the concept has undergone testing, criticism, and adjusting but it still remains one of the most widely used paradigms in the field. And although it will probably undergo further refinement, it should be remembered that it was initially proposed as an idea to test, not a definitive list of predictions to which all streams would conform. And, in this respect, it has largely succeeded. Critics should remember as Loehle (1987)pointed out that the value of theories and hypotheses may take a long time in realization and he further points out that it is dangerous to be too hasty to criticize until sufficient time has elapsed to thoroughly evaluate and test a new hypothesis.

I hope that this brief historical sketch of the RCC has been of interest.

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